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hours at 50° C., or twelve hours at the temperature of the air, and washed for a minute or two in absolute alcohol slightly acidulated with hydrochloric acid, the staining will be found to be perfect. Then follows a long description of the varying appearances of the organism when grown in the intestine, in gelatine, or on agar-agar, on coagulated serum, or in many nutrient fluids, which, although exceedingly interesting and important, is too long to be given here.

The movements of the cholera bacillus are very active, and are much influenced by the temperature, ceasing almost entirely at 16° C. They are best studied in bouillon, or liquid serum, on a warm stage. When the organism stops, there appear currents in the nutrient fluid at both ends, which seem to indicate the existence of one or more cilia. Then follows an exhaustive discussion of the various culture media, and the behavior of the cholera bacillus in them; and, by cultivations upon coagulated serum, the aërobic nature of these bacteria seems to be settled.

The rapidity of development, and the influence upon it of temperature, oxygen, and of various chemicals, are discussed at length. Cultures in gelatine, exposed to a freezing temperature for twelve days, grew at 20° – 25° C.; and cultures in bouillon were completely sterilized after two hours' exposure to 50° – 55° C. Corrosive sublimate (one part to sixty thousand of water) was destructive in half an hour to the vitality of cultures of these organisms in chicken bouillon.

The author considers that the researches made thus far prove the constant and exclusive existence of Koch's bacillus in cholera asiatica, and quotes many observers in support of this conclusion. Inoculation experiments with the bacillus under investigation, and control experiments of various kinds, were made, and are detailed at great length, all tending to show the specific nature of the bacillus.

A long discussion of the critics of Koch then follows; and the fallacies in the assertions of Strauss and Roux, Lewis, Treille, Hericourt, Finkler and Prior, and Emmerich, are exposed. The assertions of Finkler and Prior are combated at length, being the most important; and the now well-known methods of distinguishing between the organism discovered by them and that of Koch are given. [We believe that Finkler withdraws his claim of identity, and now considers the comma bacillus of cholera nostras to be only a sort of first-cousin to that of cholera asiatica. — ED.] The consequences of the discovery of Koch's bacillus are well

placed before us, and, its specific nature being granted, are worthy of the closest attention.

The most important part of the work is the study of the action of various germicides upon this bacterium. These are divided into two classes, physical and chemical; of the former, desiccation stands first in point of usefulness, and ease of application; then comes dry, and, lastly, moist heat. Of the latter, chlorine, bromine, and other gases, and various disinfecting liquids, are studied, and their action compared. Here, as in the case of other bacteria, corrosive sublimate stands at the head for destructive activity; but, owing to its poisonous properties, its use is recommended to be restricted to the disinfection of the hands, vessels, etc.; where large quantities of fluid are needed, as in the treatment of the dejecta, out-houses, etc., a five-per-cent solution of phenic acid is preferred.

The report closes with a very valuable summary of the methods of disinfection, and the strength of the solutions to be employed. Something is also said of Ferran's experiments; but these have been conducted with so much secrecy, and upon so strictly a commercial basis, that they are unworthy of attention. A number of plates accompany the report, and add to its value. Taken as a whole, the work is a credit to the author, and to the government which commissioned him to perform it, and furnishes, as far as one man can, complete confirmatory evidence of Koch's assertions in regard to the comma bacillus of cholera.

THE FIRST REPORT OF THE AMERICAN SOCIETY FOR PSYCHICAL RESEARCH.

The portion of the public that is eager for marvellous so-called 'results,' will quickly lay down this pamphlet in disgust. The first announcements and circulars of the new society are here printed together; and the chief outcome, so far, is contained in the appendix B: "Discussion of the returns in response to circular No. 4," by Prof. James M. Peirce and Prof. E. C. Pickering. And here, in the words of the committee on thought-transference in their general report, the "general result is, at present, unfavorable to thought-transference as a power belonging to mankind in general. The number of the experiments is, moreover, sufficient to cover pretty satisfactorily the particular line of inquiry which suggested them." The committee have there-

fore now recommended, in circular No. 5, an investigation of possible thought-transference in case of the ideas of geometrical forms, the first set of experiments reported on in appendix B having especially dealt with conceptions of number and color.

'Results,' we say, the sensation-seeking public cannot just yet find. But then the word 'results,' as we here quote it, is not identical in meaning with what science usually calls results. For many people, psychical research is nothing, unless it finds wonders; and by 'results' such people mean something to marvel at. But psychology is *not* concerned to find marvels; and the negative outcome of these experiments, as thus far developed, is neither disappointing nor fruitless. The existence of thought-transference of some sort has, indeed, so far been neither proved nor disproved by the work of the society. And, as was known at the outset, the range within which thought-transference can noticeably operate, has long been shown by the practical tests of daily business and social life to be at least a decidedly limited range; since, as a fact, we find it constantly possible to keep important secrets of all common sorts from curious intruders by the simple device of strict silence concerning them. Yet even the negative answer of the experiments is, so far as it goes, already a valuable answer; and, most important of all, the lines of experiment now begun already promise to prove fruitful beyond the range of the direct discussion of thought-transference.

As is shown in the report on the answers to circular No. 4, the effort to discover the existence of thought-transference in case of the number-concepts has led to the observation of certain tendencies in the mind of at least one 'percipient,' to follow certain systems of association in giving his numbers. Minute as seems at first sight the importance of such observations, it is out of just such facts that fruitful generalizations have grown elsewhere in the sciences of experience; and so it may be here. In fact, if we may venture a guess as to the future, it would be the very presumptuous conjecture, that the society may find its search for thought-transference, and for other phenomena of the mental El Dorado, as that region is now popularly conceived, a search in the end somewhat like the well-known quest upon which Saul went, just before he found his kingdom. In short,—guesses about the future results aside,—there is so much to be done for the theoretical and practical needs of psychology, so much experimental research necessary for the formation of a science that may yet

have vast influence upon the art of education, upon the treatment of the insane, and upon the policy of society towards criminals, that all experimental beginnings of such a science in any direction must be greeted with satisfaction. If thought-transference is in any mental region a fact, we shall rejoice to find it; but, if these investigations render it less probable rather than more so, they are still certain, under their present, cautious, and yet highly liberal management, to lead to other psychological discoveries that will be worth far more, very possibly, than the ones first sought. Let us hope that the members and the public will recognize more and more, as time goes on, the wisdom that led the earliest founders of the society to define its object broadly as "the systematic study of the laws of mental action." In this programme there is no sign of any unscientific limitation of work to the 'uncommon' or 'marvellous,' or 'little recognized,' phenomena of mind; although these too, when one meets with them, are to be cordially welcomed. But the society simply starts out to do scientific work without prejudice, and with scientific co-operation and patience. This first number is of course confined in its range of work; but the co-operation is well shown, and the patience in the discussion of the least exciting details is noteworthy and deeply instructive.

In addition to the mentioned papers, we find in appendix C a discussion of the "Possibility of errors in scientific researches, due to thought-transference," by Prof. E. C. Pickering. This paper treats of a test offered by the systematic observations on the magnitudes of the stars, for determining the existence or non-existence of thought-transference between the recorder and the observer. The result of the special application of this method to observations made at Cambridge, is, for the present, negative; but further application is promised. The whole pamphlet is unassuming, clearly written, and, to any sober student, helpful.

ATLAS OF PLANT-DISEASES.

THE first number of this work, by Dr. Zimmermann, consists of two folio plates, with thirty micro-photographs of different stages of three common species of *Puccinia*, together with a short notice of fungi, as the cause of diseases on plants, with special reference to the Uredineae. The text is well adapted to the

Atlas der pflanzenkrankheiten welche durch pilze hervorgerufen werden. By Dr. O. E. R. ZIMMERMANN. Halle, 1885.